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PRINCIPLES OF PLANT TAXONOMY, X*.

JOHN H. SCHAFFNER.

In previous papers the remarkable fact has been presented that the entire plant kingdom as it exists today is made up of seven progressive stages or subkingdoms with definite transition gaps of greater or less extent between them. The whole series is the result of a building up process, the higher emerging from the lower throughout the entire system. The gaps do not represent breaks in the evolutionary progression but simply the aquisition of new potentialities with a greater or less elimination of groups of plants in the transition condition from one cause or another. In some cases the geological record has supplied important missing links in others no definite transition types whatever have so far been uncovered.

In some quarters, there has been a tendency to use these breaks in the general continuity in support of fantastic evolutionary theories which would give new starting points for the various isolated groups practically all along the geological history. The fact of the non-conformity in the geological formations is supposed by some to give plausibility to the notion that there were many independent transitions from the non-living to the living. Such notions are not only fantastic but greatly increase the difficulties they would remove.

It is just in those regions of the general progressive series where the evolutionary movement compels a decided change in external life relations that the great breaks occur. Of the six transitions within the present taxonomic system of plants two, the first and the fourth, present no very great hiatus even though the new potentialities added are very effective in determining new modes of biological reactions and processes.

*Papers from the Department of Botany, The Ohio State University, No. 269.

At the appearance of sex, with the initiation of fertilization and reduction, a profound change is brought about in the method of hereditary transmission, namely Mendelian heredity, which is added to the previous mode of purely nonsexual propagation. In the fourth transition, where the time of sex determination is shifted to the sporophyte, and new biological relations are established, including decided sex dimorphisms and dimorphic reactions in various parts of the bisporangiate or monecious individual, there is also only a slight break in the continuity of forms in the various phyla which pass through this change. On the other hand, the remaining transitions involved readjustments to ecological relations and here the destruction of forms, judging from the size of the gaps, has been very great.

The great transition from the Thallophyta to the Metathallophyta involves the passage from aquatic life to a properly adjusted aerial life and also the establishment of one generation as a parasite on the other. In the transition to the vascular plants the ecological change involved is from a completely parasitic condition to a two-phased condition of the sporophyte, the parasitic juvenile phase of the individual changing gradually to an independent mature phase which finally evolves roots and prominent leaves. In the transition to the seed condition the ecological changes are again very great involving the complete parasitism of the gametophyte on the mature sporophyte, the establishment of a two-phased male gametophyte, and the intercalation of a resting phase between the parasitic and independent phases of the sporophyte. This necessitates the acquirement of very rapid readjustment reactions during the sprouting period, along with numerous other minor changes. It is no wonder that plants evolving through this progression were destroyed by the wholesale as they encountered the oscillating changes of climate and earth level during the geological ages. The transition of both the Gymnosperms and Angiosperms was from a Heterosporous Pteridophyte condition. The transition to the Angiosperms is not, properly speaking, a transition from Gymnosperms to Angiosperms but only a transition of a much greater extent and much greater advance from lower forms than that which took place in relation to the Gymnosperms. The most important change, involving ecological readjustment, was the closing of the megasporophyll, making necessary a new organ, the stigma, for the process of pollination. The closing of the sporophyll is in evidence in

various degrees in some Ferns, but in these there was no serious ecological relation involved as in the Angiosperms. In the Angiosperms not only must pollination be accomplished through a new organ, as compared with the gymnospermous condition, but the pollen tube must be readjusted to a new ecological relation, commonly involving growth through an open cavity, and, of course, growth of a much greater extent than is necessary in the Gymnosperms. These new ecological relations are probably largely responsible for the great hiatus separating our present day Angiospermæ from their nearest possible, living relatives, the Eusporangiate Ferns.

Any careful study of the taxonomic system, immediately emphasizes the remarkable fact of the great similarity and uniformity running through the whole extent of both the plant and animal series and the remarkable similarity of fundamental characteristics and properties possessed in common by the highest forms in contrast to their dissimilarity to the lowest organisms, like the lower Blue-green Algæ and Bacteria. Taking an inventory of the more important similarities exhibited by the highest plants and the highest animals, we get the following:

1. They both have essentially similar cell organization.
2. They both have the same general type of cell division (karyokinesis).
3. They both begin life as single cells.
4. They both develop into enormous, multicellular aggregates.
5. They both have differentiation systems of cells, tissues, and organs.
6. They both have sex potentiality.
7. They both have primary sexual states, causing attraction.
8. They both have essentially similar dimorphic gametes, or heterogamy.
9. They both have secondary sexual states with secondary sexual dimorphism.
10. They both produce a unitary reproductive cell from two independent units (fertilization), thus developing as diploid organisms.
11. They both have a synapsis of homologous chromosomes, resulting in a definite segregation (reduction division).
12. In consequence of fertilization and reduction, they both show fundamentally similar hereditary phenomena, namely Mendelian heredity, involving the phenomena of dominance and recessiveness and proportionate segregation of characters after hybridization.

13. They both show a general adaptation to an aerial life and dry land habitat while at the same time betraying vestigial characters indicating an evolution out of an aquatic condition.
14. They both have a parasitic interrelation of offspring to parent, the offspring developing within the protective confines of the parent.
15. They both have sperm cells which are not deposited into the water, as in lower sexual forms, but into the ovary or a special cavity of the female.
16. They both provide a food supply in one way or another to assist the offspring in establishing an independent life after the parasitic connection is broken.
17. In both the higher animals and plants there is a very decided and abrupt change from the parasitic connection to the independent life, the embryonic individual very quickly developing entirely new reactions and functions not in evidence before.
18. In the unisexual species, they both show a greater or less supply of vestigial structures of the opposite sex, indicating an evolution from a former hermaphroditic condition and the same mode of determinate orthogenetic evolution of the time of sex determination in the life cycle.
19. They both show definite correlation reactions throughout the organism as a whole together with the development of definite functional gradients.
20. They both show stability in the more fundamental group characteristics and frequently instability in small specific differences.

This array of similarities and others that might be enumerated show that the entire organic kingdom is a unit and has had a common starting point and an essentially similar mode of evolution. The lowest organisms known lack all of the things given above with the exception of a partial realization of the first characteristic and a slight evidence of the last one. As stated, since we have these profound biological parallelisms between such extreme phylogenetic lines as the highest plants and the highest animals we are impelled to accept the view of a single common origin if we are to maintain an evolutionary principle on a basis of logic and common sense rather than on a foundation of anarchistic reasoning.

The great hiatuses are then to be interpreted as breaks in the survival series rather than as breaks in the uniform continuity of the organic evolutionary process; and we may then proceed to fill out the great gaps as best we can with theoretical and fossil forms.

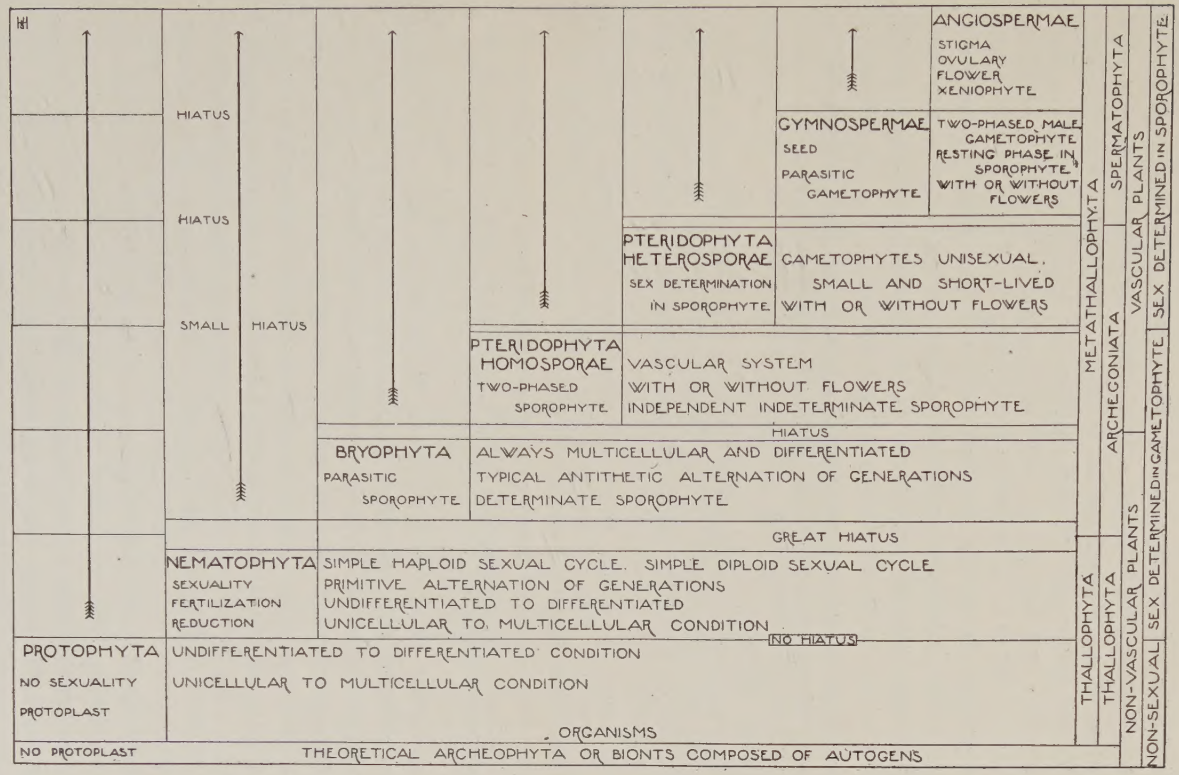

Chart showing the seven fundamental divisions or subkingdoms of living plants and the way the fundamental system is built up by the addition of potentialities to those already present.

THE TRANSITION FROM THE NON-LIVING TO THE LIVING.

We think of a time when the earth was without living beings. Non-living matter ordinarily consists of atoms and molecules, of crystals, and of coloidal particles. These, however do not show any of the distinctive qualities of life. Life is a property of protoplasm, and protoplasm consists of coloidal particles in water. In passing from the non-living to the living we are passing from the unorganized to a biologically organized system. The ultimate particles of the living system are self-perpetuating coloidal particles. The essential, ultimate living particles must have the following properties:

1. They must be autonomous or self-perpetuating, taking dead matter and reorganizing this into their own peculiar pattern with their own peculiar properties. These ultimate units are appropriately called *autogens*.
2. They must have the power of self-division after attaining a certain size, and this division is an equational division leaving the two resulting, daughter particles with the same potentialities as the original.
3. They must have the power of correlative interaction so that a number of these ultimate living units are held together in a limited or unitary reaction field.
4. They must have the power or property of intermittent mutation, by which, in isolated cases, a new pattern in one or more of the ultimate biological units or autogens is produced.

Aggregates of one or more types of these autogens would then constitute the primitive bionts, and there may have been an evolutionary series leading up to the lowest known organisms or lowest protoplasts as we have them at present in the autophytic Bacteria. Are there any such bionts, below the organization stage of the simplest protoplasts, in the world at present? Are there "species" which form an ascending ladder toward our simplest organized protoplasts (organisms)? Are disease producing plant viruses of such a nature? If these are bionts, then they need a special technique and method for their study which is neither chemical nor biological as this term applies to organized protoplasts, extending from the lowest known Bacteria to the highest plants and animals. If certain viruses are parasitic bionts, then we would expect them to pass into the contracted dry condition or spore-like state like many of the higher protoplasts and, with the return of favorable moisture and other conditions, to return to the active plasma state.

The viruses appear not to be able to perpetuate themselves except as parasites and saprophytes. This condition could not have been the beginning, as intimated above. No claim as to the nature of viruses is made here. They may be only enzymes, they may be bionts, or they may be organisms. Or known viruses might perhaps come into each of these categories. The problem however of the disease-producing viruses seems to be a problem for the cytologist primarily.

If there really are such pre-cellular bionts in existence, they would fall into the first of the ten divisions or subkingdoms of plants evolved during the geological ages and would be true Archeophyta.

Theoretically, in dealing with any sort of abiogenesis, we come sooner or later face to face with the problem of organization, if we believe in evolution at all. What is it that causes an evolution of organization, first of the protoplast itself as the unit (for there is an ascending scale from the lower Bacteria to the well organized protoplasts of all the Metaphyta). and second an evolution in the unitary reaction system of the multicellular individual?

THE TRANSITION TO THE SEXUAL, MULTICELLULAR, DIFFERENTIATED ORGANISMS.

In passing from the Protophyta to the Nematophyta, there is a broad transition belt with no prominent break in the continuity. Three fundamental potentialities arise, two of which are to be seen in their primordial evolutionary stage in the higher Protophyta. We have the evolution of the sex potentiality, the evolution of the multicellular condition, and the evolution of differentiation systems. These three do not appear in the same sequence in the various phyla and classes; so the sex potentiality is taken as the most important characteristic to segregate the first two subkingdoms, the Protophyta and the Nematophyta. The evolution of the multicellular condition and of differentiation is simply the accumulation of various hereditary potentialities which in themselves have no great consequence at the time of their several introductions. But with the introduction of the sex potentiality, which is probably the result of an advanced organization of the protoplast and especially of the nucleus, a profound change takes place in the reaction system of the entire life cycle, giving rise to the dimorphic development of the gametes with attractive

properties, resulting in fusion into a unitary system from the two independent units, and to a synapsis of chromosomes at a later period followed by reduction. As a consequence of these two processes, the Mendelian method of hereditary transmissions and interactions is established.

The change from the nonsexual to the sexual apparently was accomplished independently in various phyletic lines and was not dependent either on the multicellular condition or on the presence of vegetative differentiation potentialities. Its appearance was dependent on causes having their origin in the peculiar structure of living matter of a certain degree and type of complexity.

THE TRANSITION BETWEEN THE NEMATOPHYTA AND THE BRYOPHYTA.

The greatest hiatus in the entire plant series occurs between the Nematophyta and the Bryophyta. There is little probability that any recognizable missing links will ever be found in the fossil record to help fill the gap. For geologically these plants evolved long before the paleozoic era. It is practically certain that none have survived to the present day. It is expedient, therefore, to construct a series of hypothetical forms. In the first place the ancestors must have been filamentous green Algæ. The prominent protonema of the Bryophyta and its presence in the lower Pteridophyta fully justify such an assumption. Then this filamentous group gradually built up a multicellular constitution retaining the more primitive potentialities to be active in the juvenile part of the ontogenetic gradient. Sooner or later, with the introduction of solid tissues, plurilocular gametangia were established which by simple, direct transformations evolved into the archegonia and antheridia of the lower Bryophyta. Unicellular and multicellular rhizoids also were probably evolved before the transition and transformation to aerial conditions. The Phaeosporæ have plurilocular gametangia, but the liverworts and mosses evidently did not evolve from the Brown Algæ. They have too many special peculiarities. In a very early stage a normal alternation of generations was established which soon advanced to the typical antithetic type with a completely parasitic diploid sporophyte. No living green Alga has such a cycle. In fact it is not present in any of the living Algæ. One can see that a large number of funda-

mental morphological and ecological progressions was necessary to bring the procession up to even the lowest bryophyte stage.

Theoretically then, the great hiatus between the Nematophyta and the Bryophyta must be represented by hypothetical forms that were evolving at the same time as the higher Nematophyta. If there were any surviving representatives they would constitute a distinct and important division of the plant kingdom and would appropriately be called Protobryophyta.

THE TRANSITION FROM THE BRYOPHYTA TO THE PTERIDOPHYTA HOMOSPORÆ.

There is a large break between the Bryophytes and Pteridophytes but not nearly so great as that between the Nematophytes and Bryophytes. In this transition, the geological record gives some information and no doubt will disclose many more transition links in the future.

The main advances are as follows:

1. Change from a determinate sporophyte to an indeterminate one.
2. Change from a permanently parasitic sporophyte to an independent mature condition, only the juvenile stage being parasitic.
3. Gradual shifting of the point of origin of the sporangium from the simple terminal bud to a lateral appendage, namely the leaf.
4. Change from determinate death with the end of the reproductive process in the sporophyte, to indefinite long-continued existence not affected by the reproductive process.
5. Change from an unbranched system to a sporophyte either with dichotomous or monopodial branching.
6. Development of a vascular system and in some lines of a cambium layer.
7. Development of roots.
8. Development of leaves.
9. Reduction of the gametophyte.
10. Sinking of the archegonium venter into the tissue of the thallus, or rather recession of the differentiation reaction into the earlier stages of the ontogeny, resulting in a sunken venter with only the archegonial neck differentiating from the archegonial outgrowth.

There is no direct connection with any of the higher Bryophytes although the Anthocerotæ with their sunken archegonial venter are a closely related parallel series. We must then postulate a bryophyte ancestor with a reducing gametophyte, with sunken venter, with a tendency to enlarge the sporophyte into a

more efficient reaction system and with a definite progressive tendency to shift the reproductive tissue from the center of the axis to the outside. Since the lower Pteridophyta usually have a rather well-developed protonemal stage they are reasonably derived from the same ancestral stock, namely the green Algæ, as the Bryophytes. They passed through the bryophyte stage and were probably already segregated into the various phyletic groups long before they progressed to the present advanced position of even the lowest living Ferns. Two main phyletic lines, at least, are in evidence, the fern line with the evolution of an original cylinder of open vascular bundles and cambium, and a lepidophyte line with a more closed vascular strand, as in our present Lycopods, although these might also have had a more open bundle system until they were definitely specialized into our modern types.

As to the order in which the various pteridophyte mutations appeared, one can only guess, because of so little fossil evidence accumulated up to the present. There may have been different sequences in the several phyletic lines. The essentials, of course, were the initiation of a branching ability, the lateral movement of the spore-producing areas with accompanying indeterminateness and independence of the mature sporophyte, and the evolution of leaves and roots from the terminal gowing bud. If efficiency of meristematic growth and dichotomous branching were established before the sprangium had traveled to the margin of the stem or onto a leaf appendage, then there might be continuous vegetative growth in some axes even though others were definitely determinate because of the continuance in the race of a bryophyte type of terminal sporangium. Psilophyton appears to have been a decidedly intermediate plant, fitting into the theoretical series necessary for passing from a bryophyte stage to the lycopod condition. Psilophytons apparently were plants with vigorous growth and dichotomous branching, with no definite leaves and perhaps without roots. They still had large terminal sporangia like a moss or liverwort and these, of course, would end the growth of any branches which produced them, but other branches could continue the vegetative growth. *Rhynia gwynne-vaughani* appears to have been an even more primitive plant of the same series. It apparently had terminal cauline sporangia, vigorous growth as compared with any known bryophyte sporophyte, and dichotomous branching but no roots or leaves. Such leafless branch-

ing fossil species with large terminal sporangia are not to be compared with Algæ but with bryophyte sporophytes. Such a comparison indicates four important advances beyond the bryophyte condition as follows: (1), The evolution of long-continued, vigorous growth; (2), the acquirement of an independent mature phase of the sporophyte with the development of a rhizome-like base which could probably act as an absorbing organ; (3), a dichotomous branching system; (4), A dimorphic reaction in the terminal buds so that some developed terminal sporangia like the Bryophytes and some continued the vegetative growth.

There is no definite evidence that the fern series was derived from the lycopod line. The evidence is rather in the opposite direction. The most reasonable assumption, if the Lycopods cannot be derived from ancestral Ferns, is that both types of vascular systems were derived from a moss-like central strand of the primitive stem. Campbell has shown that there is a very direct connection between the Ophioglossales and a bryophyte ancestor something like Anthoceros. The embryology is decidedly bryophyte-like; the first leaf, stem tip and root all coming from the outer segment of the egg. Whatever the ancestral type of the lepidophyte vascular system may have been, we know that it shows the same disposition to scatter in some of the higher Selaginellas as do the bundle systems in the Leptosporangiate Ferns, Monocotyls, and various small groups of Dicotyls. It is becoming evident that at least all the vascular systems of the living plants except perhaps those of the Lepidophyta had their origin from the primitive fern type, namely the circle of open bundles with a cambium cylinder as in the Ophioglossales.

The hiatus between the Bryophytes and the Homosporous Pteridophytes represents a transition which must have contained a large number of groups in a lower stage of evolution than our present Lycopods and Ferns. Rhynia and Psilophyton represent transition forms between the Lycopodiaceæ and their bryophyte ancestors while no ancestral Ferns of the same level are known at present. But fern-like ancestral forms with terminal and perhaps lateral cauline sporangia must have existed. The fern series never evolved a dichotomous branching system and the ancestral groups must either have had unbranched but vigorous sporophytes with an independent mature phase or else they were evolving a rudimentary monopo-

dial branching potentiality. All such plants whether ancestral Lycopods or Ferns were the true Protopteridophyta.

The lowest vascular plants in which the sporangia had traveled from a terminal position to a lateral position on a lateral appendage or leaf probably had alternating zones of sporophylls and foilage leaves just as soon as indeterminate growth was fully established as is the case in all flowerless fern types and in the typical flowerless living Lycopods. For it is a general characteristic of primitive ontogenies to have rhythmical repetitive reactions rather than irregular sequences. Such rhythmical reactions depending on developing gradients, are seen not only in the alternation of zones of sporophylls and foliage leaves but also in the rhythmical appearance of branches or branch whorls on the main stem alternating with branchless zones, as in Araucaria and other Conifers and some species of Magnolia. Any species then which was thrown from the juvenile vegetative condition with foliage leaves to a permanent condition of sporophyll production, until the stopping of growth of the reproductive branches was attained, would indicate a first slight movement toward definite strobilus or flower evolution. In the most primitive condition the sporangia would not be on the leaves but directly on the axis, so systems could develop in which small leaf whorls would alternate with whorls or zones of sporangia.

THE TRANSITION FROM HOMOSPOROUS TO HETEROSPOROUS PLANTS.

For some unknown reason, after the sporophyte has attained a certain degree of advancement in the various phyletic lines, the point of sex determination passes from gametophyte to sporophyte. The general reason is that the orthogenetic progression can continue until the zygote is reached, and this progression is in evidence in many higher phyletic series. The movement is a process in which the time of sex determination continues to gain on the ontogeny until the determinate limit is reached in the fertilized egg or zygote where this evolutionary advancement necessarily comes to an end in the attainment of diecious species. The fact must be emphasized that the transition from sex determination in the haploid gametophyte to the diploid sporophyte is not dependent on the simple or double number of chromosomes nor to any special kind of morphology. It took place in series that had the lepidophyte

morphology as well as in series that had the ptenophyte and calamophyte morphology. It took place without reference as to whether a flower had evolved in the system or not. There was a great destruction of forms of plants which had arrived at this stage of evolution. But this is not to be ascribed to the change in sex condition but rather to the increasing helplessness of the gametophyte which at this stage went hand in hand with the evolutionary progression in the time of sex determination. Some of the missing links leading from the Homosporous Ferns to the Cycads have been discovered, namely the Pteridospermæ, but not much progress has been made in uncovering missing links which must have been left behind on the path leading up to the Strobilophyta. There are also very few fossil links known which might aid in solving the problem of the vast isolation of the Anthophyta.

THE TRANSITION TO THE GYMNOSPERMS.

The great advance which culminated in the perfection of the seed was certainly one of the major events in the evolution of organisms. The main movements were, of course, the making of a complete parasite out of the gametophyte; the gradual intercalation of a resting phase, often followed by a long period of dormancy, between the parasitic embryonic phase and the independent mature phase of the sporophyte; and the two-phased parasitic growth of the pollentube. Each of these changes required decided ecological adjustments. The lower Cycadophyta are flowerless plants, like the Ferns, with simple rosettes of sporophylls, while most of the advanced forms attained to the strobilus stage, acquiring a definite, determinate, reproductive axis, as is prominently shown in the higher Cycadeæ. In the Ginkgo series, however, no flower was evolved, although the other important morphological systems advanced to a very high condition. The gymnosperm strobilus is of the same general nature as that which appeared in the Lycopodium and Equisetum lines. It is perfectly evident that the flower had a large number of independent origins, just like the origin of heterospory or various other important, orthogenetic, evolutionary advances, as for example the development of dimorphism between sporophylls and foliage leaves and the attainment of dieciousness. In all the various cases, however, the evolutionary process was the same; namely, the introduction of a determinate potentiality active in the reproductive shoot and

the continued movement of this potentiality toward a more definite reaction and a more prompt determination in relation to the ontogenetic gradient of the floral axis.

The primitive types of flowers are represented in Lycopodium, Equisetum, Selaginella, Cycadeoidea, Encephalartos, Araucaria, Magnolia, Ranunculus, and Echinodorus. Extreme types are present in Taxus, Gnetum, Naias, Typha, Wolffia, Corallorrhiza, Monolepis, Peperomia, Casuarina, Salix, Hippuris, Xanthium, and Taraxacum. The attempt to evolve a flower from an inflorescence must be regarded as fantastic and based on anarchistic modes of thought. If one takes an inventory of the acquirements of complex systems of potentialities possessed by the entire plant, namely the complete compliment of hereditary factors, of any of the extreme types mentioned above, it will be found that the species with reduced flowers have the most complex reaction systems, and that in them the various evolutionary progressions which have been operative from the beginning are also the most advanced in the species which have the most extreme flowers. The various evolutionary movements, although they are mainly independent of each other, show a remarkably consistent, forward progression.

When one makes a study of such a classification as that developed in the Engler system or the proposals put forth by Wettstein and others, where a Magnolia, for example, with its decidedly simple and rhythmical reaction system, is derived from the Gnetales, with their enormous complement of extreme specializations, and evolved through such forms as Casuarina, Quercus, Juglans, etc., or their immediate theoretical ancestors, then the whole scheme appears not only fantastic but the extreme of absurdity.

In the lower Homosporous Pteridophytes there were no doubt series evolved with alternating simple zones or whorls of sporophylls and foliage leaves, or in some cases with lateral cauline sporangia subtended by foliage leaves or bracts. One would expect such types from the fact that the spore-producing activity travels out from the center of the primary stem to the leaf appendages and in some cases even to the epidermis of the leaves; and after a branching system had evolved it often is shifted entirely away from the primary axis to secondary or tertiary axes and their appendages. These special, primitive systems present in some fossil plants give no excuse for pre-

tending to see a reducing, branched inflorescence when the evolution of branching whether dichotomous or monopodial had hardly begun.

The Strobilophyta are much more isolated from the Pteridophyta than the Cycadophyta but not nearly so much as are the Anthophyta. The general evolutionary movements are the same as in the Cycadophyta, only on a more advanced and complex scale. The Araucariales are plainly the most primitive conifers, and from the lowest broad-leaf Araucariaceæ the evolution of the leaf is consistently carried forward to the extremely reduced scale and needle-leaf types. The flower also evolved consistently in the same direction and along with it there is a parallel advance of numerous other characteristics.

Conifers show no evidence of a previous, bisporangiate condition of the flower. But in all three orders, Araucariales, Pinales, and Taxales, both monecious and diecious species are present. Although the movement in the evolution to unisexuality is not closely associated with or dependent on any other evolutionary advance taking place in the plant body, yet there is sometimes a parallelism of evoluting movements as would be expected if evolution is progressing in definite directions. In the Pinales dieciousness is only attained in the most extreme genus, Juniperus, some species of this genus still being monecious and some diecious. In the Taxales which show a consistently higher level in nearly all evolutionary movements, dieciousness is attained quite a number of times in independent genera.

The problem of connecting the Conifers with the Ptenophyta is to find fossil missing links, especially Heterosperous Ferns, but the question arises as to whether these would be recognized as ancestral Conifers even if discovered. The connecting links would be broad-leaved (not necessarily compound) trees; the line finally extending below the level of the presence of flowers and probably of branches.

In passing downard to theoretical ancestors, two conditions are possible for a derivation of monecious and diecious flowers as represented by present Conifers. Differential gradients determining one or the other sexual states in any reproductive bud may have been established before the mutation to determine reproductive axes took place, just as we have monecious flower systems in which one or the other sex condition is temporarily established in the primary axis from which the secondary

floral axes branch off. Then with the introduction of reproductive determinateness monosporangiate flowers would naturally follow directly. On the other hand, undiscovered bisporangiate flowers may have preceded the monosporangiate condition, which would lead back to indeterminate reproductive zones with one sexual state present at the beginning stage of development, and through a sex reversal, with the opposite sexual state at the end of the ontogeny of the reproductive zone. Then the introduction of reproductive determinateness would necessarily give bisporangiate flowers. The lack of vestigial structures of the opposite sporophylls is against this hypothesis and would indicate that the conifers never had bisporangiate flowers. The same reasoning may be applied to the living Cycadales in spite of the fact that their relatives, the Bennettitales, had bisporangiate flowers.

The Gneteæ, on the other hand, like many monecious and diecious Angiosperms, plainly had ancestors with bisporangiate flowers, since some of them show definite vestigial structures of the opposite set of sporophylls.

THE TRANSITION TO THE ANGIOSPERMÆ.

As stated previously, the Angiosperms or Anthophyta, did not have their origin in any of the living groups of Gymnosperms. At most the nearest approach to a common ancestry of the two groups might be sought in hypothetical Heterosporous Pteridophytes which would trace back to Eusporangiate Ferns, where both open and closed vascular bundles evolved, comparable to the open and closed bundles of the Dicotyls and Monocotyls. In various Leptosporangiate Ferns there is a strong tendency to close up the leaflets of the sporophylls. This attains extreme development in the Marsileales. Now we will have to assume that somewhere along the line the ancestral Angiosperms passed from the open to the closed megasporophyll. Since the stamens were apparently not involved in the movement it probably did not occur until the group was passing through the Heterosporous Pteridophyte stage or to the seed-bearing condition.

If there were already distinct phyletic lines of ancestral Angiosperms established, then all the lines passed through the same progression. And this is the usual occurrence in all fundamental evolutionary movements. When it comes to general growth habit and form, many of the Monocotyl trees

are much nearer the primitive Fern type than any Dicotyls and there is no special reason for supposing that they did not come through the last three subkingdom stages in substantially the same form.

The vascular system of the Monocotyls probably segregated off long before one could speak of Monocotyls and Dicotyls in the modern sense. The same movement developed to a slight extent in many lines of Dicotyls giving scattered and partially closed vascular bundles in many, mostly decidedly advanced Dicotyl groups. Thus, so far as the vascular system is concerned, the "Monocotyls" probably evolved from the "Dicotyls." But when it comes to embryonic structures the evolution was plainly in the opposite direction. The primitive Ferns have one "cotyledon" and the Monocotyls have simply continued this condition, with various minor modifications, while the Dicotyl embryo represents a decided specialization which also appeared, probably independently, in the Cycadophyta and Strobilophyta. The Dicotyl type of embryo advanced in specialization in various Strobilophyta until a whorl of numerous cotyledons was attained. On the other hand, the Dicotyl type of embryo may be transformed into a "Monocotyl" type by the introduction of a factor which suppresses one member, as in Claytonia. The notion that the Monocotyl condition was derived from the Dicotyl by fusion is to be considered only as a survival of an archaic method of morphological reasoning which thought of the plant as being built up of distinct parts rather than as developing through the reaction of hereditary potentialities which were successively evolved and through which the reaction system is changed at the right point in the ontogenetic cycle provided a proper environment is at hand. We know that a change to a suitable environment will often cause a new reaction and bring out structures which never could have appeared in the normal sequence of the phylogenetic history.

The flowers of the Angiosperms are plainly and conclusively strobili, as stated above, of the same general nature as those of Equisetum, Lycopodium, Dioon, and Araucaria. They are determinate, reproductive axes with ordinary, modified sporophylls. They were all of a single primary bisporangiate type originally and the indication is that they evolved about the time when heterospory was attained. From the bisporangiate condition they advanced to the various degrees of moneciousness

and dieciousness while at the same time a general evolutionary movement was going on producing a more definite and an earlier action of the determinate potentiality. That the primitive Angiosperms were entomophilous without special adaptations is hardly open to question. Various Ferns, like *Pteris aquilina* for example, have abundant nectaries on their leaves and these are visited by various insects which gather the abundant secretions while the leaves are expanding. No one at present would continue to put forth the foolish teleological claim that such visits were a life and death advantage to the plant or any advantage at all. Some of the Angiosperms had the nectaries from the beginning and some perhaps did not. The presence of an insect environment could not determine the origin of nectaries, although in the presence of an insect environment specializations may evolve which will cause elimination whenever this special insect environment is absent.

With the introduction of branching potentialities and reductions, inflorescences were finally evolved which are of the same general nature and due to the same causes as the inflorescences one finds in the higher Lycopods, in some of the Taxodiaceæ, and in the Gneteæ. The development of a perianth and of a peduncle is again of the same general nature as the development of peduncles or perianths in Equisetum or of peduncles and specialized basal sporophylls in certain Lepidophyta, Cycadophyta, and Strobilophyta. The Angiosperms, developing a definite bisporangiate condition, with the stamens below, obtained a perianth mainly by the transformation of stamens. As special potentialities were added that came into play mainly when the ontogenetic gradient was passing through the zone from vegetative determination to reproductive determination in the flower, the stage was set for special developments on this transition zone, resulting in peduncles and perianths. Neither the Engler system nor Wettstein's speculations can be taken seriously by any one who has advanced along modern lines of discarding teleological evolutionary theories and who thinks of evolution as an orderly mutative process, to account for the taxonomic system; for this system shows the most remarkable orderliness not only in the great fundamental progressions from the most simple to the most complex organisms but also in the sequences of the subordinate phyla and smaller series.

When the claim is made that the Magnolia flower represents the primitive type of flower for all Angiosperms, we are not claiming an immediate monophyletic origin of all existing Angiosperms, except in the broadest sense, any more than when we claim that the strobilus of a Lycopod and the strobilus of a Cycad are exactly homologous structures do we intimate that these two diverse groups had an immediate monophyletic origin. The Protangiosperms must have been evolving as a distinct phylum side by side with the Gymnosperms and at the same time.

There is but one rational view in respect to the evolution and phylogeny of the Anthophyta. They connect with the lower members of the great mother phylum of all the living seed plants, namely with the Ptenophyta. They evolved strobili of the same general nature as the Equisetaceæ, the higher species of Lycopodium, the Selaginellaceæ, the Cycadaceæ and the lower Coniferæ. The lowest living Angiosperms, as stated above, still have these strobilus-like flowers with very moderate modifications, in fact with modifications so slight that one can make a direct comparison with the strobili of Homosporous Pteridophytes. These flowers, in fact all flowers, consist of a main determinate axis bearing sporophylls. In some cases modified sporophylls appear even in very low types; in others there is a direct succession to the foliage leaves below. The flowers are derived from indeterminate axes through mutative processes which involve the development of functional gradients, these gradients not being subject to rejuvenation or repetition normally as in the lower stage where rosettes or zones of sporophylls alternate with the growth of vegetative leaves. The retention of the primitive condition is represented among living higher plants in the carpellate rosettes of Cycas and the carpellate and staminate rosettes on the indeterminate dwarf branches of Ginkgo. From the primitive strobilus type of flower have evolved all the diversified flower types in both Angiosperms and Gymnosperms and the sequence to the extremes can easily be followed out in a host of phylogenetic lines. The evolution resolves itself into a few simple, fundamental movements which were practically the same in all the series.

The botanist who deserves credit above all others for laying the foundation of modern plant phylogeny on a correct and rational basis was Dr. Charles E. Bessey and the writer has

often marveled that those who were supposed to have some intimate knowledge of systematic botany, both in America and Europe, could not see the way after it had been pointed out by Bessey, but persisted in phylogenetic speculations and taxonomic arrangements which make a travesty of any rational notion of plant evolution whatever. When the series are in proper order we discover a remarkable system and the actual movements of the evolutionary process become evident. With improper arrangements, like some which are still in high repute and in very general use, the whole system appears hap-hazard and the product of the whims of witches rather than of a rational order of law. One can be sure, however, that the disorder is in the minds of the taxonomists and not in the plant system itself.

I am greatly indebted to Dr. Lois Lampe for the execution of the accompanying chart, which illustrates the remarkable orthogenetic accumulation of the fundamental properties and potentialities in the plant kingdom.

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HABITS OF THE SHORT-TAILED SHREW, BLARINA BREVICAUDA (SAY).

W. J. HAMILTON, JR.

The following notes are presented in the hopes that they may be of interest to students of the smaller mammals. The observations submitted here have been made over a period of six years, commencing in 1924 and continued as time permitted.

MOLTING.

Molting appears to take place without regard to season, and specimens taken in December and June alike show changing coats. The most pronounced change from winter to summer may best be observed in May and June, while specimens taken in November will usually show some indication of a change to winter pelage.

The short summer coat usually makes its appearance on the back, slightly caudad of the shoulders. From here it proceeds backward and forward in no particular manner. Full shedding occupied approximately six weeks in two captive animals.

WEIGHTS.

Average weights of 53 adult males from Ithaca, N. Y., 20.35 gms. Average weights of 27 females from Ithaca, N. Y., 18.3 gms. It would seem that weight is a fair indicator of maturity in this animal, when coupled with the measurements. The health of the individual and the amount of available food are potent factors in determining weight. However, specimens under sixteen grams were never found to be mature. The heaviest shrews secured were two males, that weighed 27 gms. and 26.3 gms., respectively.

RANGE OF THE INDIVIDUAL.

The home range of any mammal is a difficult thing to determine, more so with the smaller forms than with the larger. The range of any Blarina is probably restricted to an acre of ground, while the ranges of two pair or individuals most surely overlap at all times.

As to the numbers, some light may be thrown on this by systematic trapping. The numbers will of course vary with the environment. Where one woodlot will yield upward of a hundred an adjoining field will contain perhaps a single pair. Various writers have endeavored to determine this point. Seton (1909) says:

"After a fall of snow I walked for a mile through the woods at Cos Cobb and found labyrinths of fresh Blarina tunnels every fifty yards. There was doubtless a Blarina for every system of tunnels, and many of the species were probably not yet represented, as it was but three or four hours since the fresh snow came. This then, affords a minimum gauge of the creature's numbers. In these woods, there were certainly fifty Blarina's to the acre."

Shull (1907) estimates their numbers at four per acre. Brooks (1908) regarding their numbers, says:

"In less than a week the traps caught over twenty short-tailed shrews under a single chestnut tree."

This can not be considered a good normal index of the shrew population, the shrews being attracted to the weevil larvæ in the chestnuts, as Brooks subsequently points out.

In a tract of woodland where the short-tailed shrew was exceedingly abundant, the writer took nineteen in one night from fifty traps. Trapping was confined to ten yards each side of a path and one hundred and fifty yards along it. In three nights the total catch amounted to thirty-seven. This area totaled 27,000 square feet or about three-fifths of an acre. This gives very little indication of the numbers to the acre in this particular woodland, as the shrews' individual range is not known. It is very likely that these individuals ranged over an area many times as great as the boundaries of my trap line.

In one trap set in a small swale I caught eleven shrews in fifteen nights. Again, in a hilly tract, sparsely wooded, eight Blarina's were caught in a single runway in seventeen days. On another occasion three short-tailed shrews were caught in the same runway in twenty-four hours.

Collecting in a wooded tract that appeared to be favorable to these creatures, I placed out fifty traps and attended them daily for a week. The total catch resulted in two shrews and two deer mice.

From these observations and more made while collecting the writer should judge that there could exist about four pairs of shrews to an acre in a choice locality.

PERIOD OF ACTIVITY.

Blarina is active both by day and night. On several occasions I have seen this shrew, in the wild state, moving about during the brightest hours, but invariably in subdued light or shade. Captive specimens do not avoid light that is not too bright, but cannot stand the bright rays of the sun for more than a few moments without fatal consequences. One which I had captive, when placed in direct sunlight, became at once uneasy, and in a minute appeared very much agitated. It ran about ceaselessly, attempting to find cover, and at the end of four minutes, lay on its side, breathing very hard. Another which I had died in less than ten hours, due, I believe, to the very bright enclosure in which I had placed it. It had no cover.

Over thirty specimens have been taken in open runs between the hours of 7 A. M. and 5 P. M. and examination of the stomach showed they had fed during this time.

While setting traps in a rather dense copse in late May I saw one of these little animals running among the dead leaves at the side of a fallen log. The hour was close to noon and the sun shining brightly, but somewhat diffused by the thick growth of alders. The shrew was apparently hunting insects, as he repeatedly picked up small morsels. A very low chattering, not unlike the faint squeaking of bats, was heard, and this was kept up continually while the animal was under my observation.

Shrews must be fairly sound sleepers. When coiled up in their box, asleep, the lid was raised and no heed paid to it. Even when lightly touched with a pencil they simply stirred slightly.

DISPOSITION.

Are shrews solitary or sociable animals? All the written evidence would point to their being extremely solitary. The oft repeated observation of Merriam, who placed three *Sorex* under a glass, where they immediately fell upon one another, would possibly bear this out. But Brooks (1908) indicates they are not adverse to one another's company when he says:

"On April 28, 1906, while collecting insects in a woodland near Morgantown, W. Va., I found an open sunny spot where I quietly stood for some time to sweep for passing butterflies. While thus engaged, a rustling in the leaves nearby attracted my attention and approaching noiselessly the part from which the sound proceeded, I saw three short-tailed shrews running about over the leaves, evidently foraging for insects. They were hunting within a few feet of one another, and several times were seen to rush about excitedly, as though in pursuit of some lively game."

Two captive males which I placed together were rather nervous, but this may be attributed to their close confinement. When one touched the other, the latter simply backed off. Indeed, they showed only indifference to one another. Large numbers caught in the same runway over a short period of time would possibly point to their toleration of one another. It appears certain that the male and female are much together during the pre-breeding season.

LOCOMOTION.

Blarina is such a short legged creature that little speed can be made. It usually runs, the tail invariably elevated, much after the fashion of the long-tailed soricids. Very rarely do they jump, yet tracks in the snow sometimes show where the gait changes from a run to a series of short leaps. Captive shrews rarely exhibited this tendency, but on one occasion, when I attempted to lift the nest box containing young, the mother leaped from the floor of her enclosure to her nest box opening, a distance in vertical elevation of about three inches.

The speed is less than *Microtus*. A very slow walker can readily overtake a shrew. They are incessant runners, and never seem to tire, appearing to be embodied with a restless spirit that keeps them forever on the go.

The mole shrew is well adapted for digging in loose soil. The fore paws are wide and strong, slightly larger than the hind feet. In digging, the fore feet are used to start the burrow, but the head and nose soon take up the burden of this task.

Shull (1907) remarking on the digging habits of this animal, says:

"In this soft soil, the shrew at once thrust its nose, and by violent backward and outward strokes of its forefeet, forced its way through the soil like a wedge. No difficulty was experienced in burrowing 20–30 cm. in a minute."

RUNWAYS.

The burrows and tunnels of this shrew vary considerably. They are from one to two and a half inches in diameter, rarely more. There appear to be two zones, one a few inches below the surface or directly upon it, and one at a considerably deeper level. These two levels are joined at irregular intervals by abrupt connections. The deeper runways will descend from sixteen to twenty-two inches below the surface, but it is uncommon to find where a shrew has ventured lower than this.

Some runs are more frequented than others. For example, certain well-worn runways near the surface of the ground yielded eleven and eight shrews respectively in a fortnight of trapping, while not infrequently others would yield from four to six in a week's time. Others close by would yield a single pair, and continued trapping in the same place would fail to take more. The two runways, regardless of yield, appear little if any different from one another. Often the runs will follow just beneath an old log, sometimes penetrating it when it is rotten enough. Such logs may be fairly honey-combed with the runs of this species, and on one occasion I found the resting nest of this shrew in such a situation.

These animals resort to the runways of meadow mice not infrequently and also are occasionally taken in the wider tunnels of the star-nosed mole and Brewer's mole.

SENSES.

Most scientists are in accord with the statement that sight plays little or no part in aiding the shrew in its hunt for food, the eyes merely allowing it to distinguish between light and darkness. But Brooks (1908) speaking about this species in the wild state, remarks that:

> "The shrew made several frantic but well directed leaps for the cricket, caught it, and immediately darted out of sight beneath the leaves. The precision with which the shrew jumped towards its prey led me to suspect that the sense of sight is more perfect than the dimunitive eyes would indicate."

Ryder (1888) made a careful study of Blarina's eye and concluded from a study of the ocular muscles that the eye is capable of adjustment for the direction of vision.

On one occasion I introduced a jumping mouse into an enclosure with a shrew. The shrew made several accurate

leaps at the agile *Zapus*, which was always too swift for the shrew. It certainly seemed as if the shrew saw the mouse on this occasion.

The eye is clearly seen when the animal is active, and I certainly think it has greater power than to simply distinguish light from darkness.

Apparently the sense of smell is developed to an average degree. Freshly killed mice, placed six inches from the floor of the shrew's enclosure, apparently baffled the animal. It repeatedly passed underneath the suspended rodent but the sense of smell was not sufficient to direct it to the dead animal. Shull (1907) however, believes that shrews distinguish between empty and occupied snail shells chiefly by their odor.

Hearing is perhaps better developed than the other senses. Many writers allude to the exquisite hearing. One which I was observing in a wooded thicket immediately ran to cover when I made a slight movement. Whether the noise of my foot, or the vibrations caused by it, alarmed the shrew, I am not in a position to say.

VOICE.

In general, the calls of shrews and bats are strikingly similar and one would think of the two orders these animals represented as being very close if voice plays any part in their classification. Dice (1925) speaks of it as "a high musical twitter" while Hahn (1908) says it is almost exactly like the rapid chip-chip-chip of the ground squirrel. Miller (1897) states that when angry, the shrews utter a sound much resembling a red squirrel chatter greatly reduced in volume, while Kennicott (1857) adds of hearing a short clear cry, the voice calling to mind that of the mink, but softer and lower.

A peculiar call, oft repeated when disturbed, was heard from captive specimens. This commenced with a shrill grating note, rather high pitched and ends with a slightly lower pitched chatter, like zeeeee-che-che-che-che. Another note, apparently of contentment, is sometimes heard when the animal is feeding, and is entirely distinct from that of alarm or rage. This is very low and continuous, calling to mind the drowsy twitter of sparrows. The voice is developed at an early age, the young giving a sucking note when eight days old, and capable of a tiny chatter at twenty days.

ENEMIES.

Internal Parasites.

Cestodes were found in the greater majority of intestines examined. One tapeworm was as long as the shrew it was taken from. Ripe proglottids often fill the large intestine and extend it.

Nematodes are very abundant. Small eelworms, orange in color and measuring 16-20 mm. are particularly abundant in the stomach and duodenum, sometimes filling the pyloric orifice and almost closing this opening at times. It is obvious that the great abundance of this ascarid must cause some discomfort and possibly death.

Other roundworms occur just under the integument of the rump and shoulder region, imbedded in the mesenchyme. These worms are enclosed in a tissue sheath, and always coiled. They measure 3-4 mm. in diameter when coiled and average 35 mm. in length when outstretched. There may be twenty to thirty on a single animal.

External Parasites.

Acarinids from Blarina have been determined by Dr. H. E. Ewing. *Campylochirus*, *Parasitus*, *Myobia*, *Liponyssus*, *Laelaps*, *Haemogamasus* and *Ichoronyssus* are the genera determined. Generally mites are found about the inguinal region. Fleas are prevalent on this shrew. Among the species determined are the following: *Ctenophthalmus pseudagyrtes*, *Ctenophthalmus wenmanni*, *Ceratophyllus wickhami*, *Doratopsylla blarinæ* and *Nearctopsylla genalis*.

Predatory Animals.

Among fish, a northern pike, *Esox lucius*, was found to have this shrew in its stomach by the authors of "Fishes of Lake Nipigon." Mr. John Meyer of Ithaca, N. Y. caught an eleven inch rainbow trout that had one of these animals in its stomach. Undoubtedly all predaceous fish would not resist a shrew if opportunity offered.

Among the reptiles, Surface (1906) records shrews from the stomachs of the spotted water snake, pilot snake, rattlesnake and copperhead. In most instances the shrews are not specifically identified. Dr. J. R. Greeley found this shrew in the

stomach of a milk snake. A black snake which I opened contained a *Blarina*.

Raptorial birds take a large toll of shrews. Fisher (1893) records the short-tailed shrew from the stomachs of the following: Red-tailed Hawk, Red-shouldered Hawk, Sparrow Hawk, Broad-winged Hawk, Barn Owl, Short-eared Owl, Barred Owl and Great-horned Owl. I found seven *Blarina* skulls in the pellets of a breeding pair or Long-eared Owls. Both the pellets and stomach contents of Barred and Screech Owls have yielded shrews.

Among mammals, Brooks (1908) found the most conspicuous thing about the den of a fox was the dead bodies of the short-tailed shrew. He did not count them but says there must have been twenty or thirty in sight. In the stomachs of *Mustela noveboracensis* and *M. cicognanii* I have determined the mammalian remains as those of this shrew. Among a large number of skunk stomachs examined by the writer, two contained parts of *Blarina*. It is likely preyed upon by a host of others, but whether habitually eaten seems unlikely.

PERIODIC MORTALITY.

Not a few observers state that a decided mortality occurs in this species during the fall. Large numbers are found dead, some without apparent injury. Everman and Clark (1911) record twelve specimens from Lake Maxinkuckee, nine of which were found dead, and all in the month of October.

The same situation is met with in England. Some authors hold that insufficient food is the cause of their death, some that lack of water causes the high toll, while a few attribute the high death rate to predaceous animals, which, finding the shrew distasteful, have dropped it where caught. Adams (1909) writing on this phenomena in England, gives as his impression that the annual loss in the fall is due to nothing more than old age, this being reached in the Common and Lesser Shrew in, roughly, thirteen or fourteen months. This is based solely on the fact that all immature shrews are taken after December.

If there is a decided increase in the death rate of *Blarina* during the autumn, and my observations do not point to this, it may be simply due to the fact that the dead specimens are more readily seen at this season.

Internal parasites, particularly roundworms, appear to be more prevalent or larger at this season, and possibly may be an indirect means of the animal's death at this season. Some specimens examined were so parasitized, especially in the pyloric region of the stomach, that it was a wonder to the writer how food could pass by the obstruction caused by these nematodes.

ECONOMIC IMPORTANCE.

Most writers are in accord in ranking this shrew high as a direct aid in keeping down the host of injurious insects and mice. Shull has, I believe, greatly over-rated their importance in holding in check the meadow mouse. He has based the food eaten by these shrews on the relative abundance of various items in the swamp region studied. Shull did not know the predilections regarding food these animals exhibited in the wild state. He estimated four shrews to an acre, and decided that on a farm of one hundred acres, these shrews would account for 38,400. But the writer's studies (Hamilton, 1930) of the food of Blarina show that of 244 specimens examined in the flesh, only 4 contained mouse remains.

It is my opinion that the economic importance that has been credited to shrews has been greatly exaggerated. Shrews seldom occur in numbers sufficient to be of any real significance in cultivated areas. This animal usually prefers a habitat unsuited to crops, and while they may devour insects in large numbers, they certainly do not distinguish between noxious and beneficial forms. It is true that in a given locality they may be a potent factor in overcoming a small outbreak of a dangerous pest, but this is exceptional. This shrew is said to be the principal enemy of the larch sawfly in New Brunswick, and is credited with eating forty per cent of the cocoons. Brooks has shown that it may check the chestnut worm (*Balaninus*) in a small locality.

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LeCONTE'S EPITOME OF EVOLUTION

George J. Dudycha
Ripon College

As has been pointed out by various writers, the history of the evolution idea is not a brief one. Many thinkers—the ancient Greeks, the early theologians, the eighteenth- and nineteenth-century philosophers and biologists—have directed their attention to the difficult and unsolved problem of the origin and development of life, of society and of the universe. As we trace through the history of thought, with the light which science has given us, we observe that some of the great thinkers who have wrestled with this problem of development have made contributions of inestimable value in spite of their meager scientific knowledge and inadequate observations; others, due to the same limitations, submerged their more valuable thoughts in a host of naïve ideas; still others made no advances but merely summarized and organized the ideas and suggestions of those who preceded them. As Lucretius summed up all the teachings of the non-Aristotelian philosophy, so LeConte epitomized the major pre-twentieth-century ideas concerning evolution. Let us, then, examine LeConte's idea and see what there is in it of value.

In the first chapter of his book, *Evolution**, which first appeared in 1887, LeConte deals with the scope and definition of evolution. In order to objectify his notions, he refers again and again to the development of the egg as the type form of all evolution.

> Every one is familiar with the main facts connected with the development of an egg. We all know that it begins as a microscopic germ-cell, then grows into an egg, then organizes into a chick, and finally grows into a cock; and that the whole process follows some general, well-recognized law. Now, this process is evolution. It is more—it is *the* type of all evolution. It is that from which we get our idea of evolution, and without which there would be no such word. Whenever and

*Joseph LeConte. *Evolution, Its Nature, Its Evidences, and Its Relation to Religious Thought*, (Second Edition, Revised), 1899. New York: D. Appleton and Company.

wherever we find a process of change more or less resembling this, and following laws similar to those determining the development of an egg, we call it evolution.*

LeConte, like Spencer, says that evolution, as a process, is not confined to the development of one thing, as the egg, nor as a doctrine is it confined to one science, as biology, "The process pervades the whole universe, and the doctrine concerns alike every department of science—yes, every department of human thought." All things may be studied from two points of view. From the one view we are concerned with things as they are; but from the other, with the process by which they become what they are. "This 'law of becoming' ", says LeConte, "in all things—this universal law of progressive inter-connected change—may be called the law of continuity." This law asserts that there is a universal causal relation between things in time. "This is the universal law of evolution." Thus evolution is a single process which unifies and binds together all things into a causal and temporal relationship. Although LeConte does speak of evolution as being universal, he seems to limit his discussion and thinking almost exclusively to biological evolution. "If there be any evolution, *par excellence*, it is evolution of the individual or embryonic development."

Now that we have an idea of LeConte's general approach to the problem of development, we are ready for the statement of his definition which is as follows: "Evolution is (1) continuous *progressive change*, (2) *according to certain laws*, (3) and by means of *resident forces*."† Since he takes embryonic development as his type for all evolution, he assumes that everyone will admit that his definition is completely realized in this process of development. "The change here is certainly continuously progressive; it is according to certain well ascertained laws; it is by forces (vital forces) resident in the egg itself." Since, then, this definition applies to the type form, he concludes that it applies equally well to all things and hence is a valid definition.

Since we are not satisfied with the mere statement of his definition, let us analyze it carefully and see what he means by each of the elements which it embraces. First, what does

*LeConte: *Evolution*, p. 3.
†Loc. cit., p. 8.

LeConte mean by *progressive change?* We shall best understand his meaning from his own statements. He discusses this first element of his definition in three paragraphs and illustrates his idea by the Ontogenetic series, the Taxomonic series, and the Phylogenetic series. Of the first he says:

"Every individual animal body—say man's—has become what it now is by a gradual process. Commencing as a microscopic spherule of living but apparently unorganized protoplasm, it gradually added cell to cell, tissue to tissue, organ to organ, and function to function; thus becoming more and more complex in the mutual action of its correlated parts, as it passed successively through the stages of germ, egg, embryo, and infant to maturity. This ascending series of genetically connected stages is called the embryonic or *Ontogenetic* series."*

Similarly, he discusses the Taxonomic series and the Phylogenetic series, and in a sentence closes that section by saying: "It will be admitted, then, that we find *progressive change* in organic forms throughout geologic times." Does this make clear to us what "progressive change" is? It is true that in the individual we have the stages of germ, egg, embryo, infant and maturity. Also in examining the rock strata, we find simpler forms of life in the lower levels and more complex in the higher; but a mere statement of these facts does not explain to us what progressive change is. What is meant by change which is progressive? LeConte makes extensive use of the analogy of the branching tree of life in which he represents progressive change by the branches which go upward. Manifestly to say that progressive change is change upwards means nothing. If, however, he means by *up* increasing complexity, then his statements bear a little more meaning. But is all change in the direction of increasing complexity? Is increasing complexity necessary to change before we may call it evolution, or may we call any process of change evolution? These are some of the questions which confront us, and which LeConte does not answer.

When we study the earth's history we find, as we progress through the Eozoic, Palaeozoic, Mesozoic, Genozoic and Psychozoic periods, there an indication of a progression in life from simple to complex. Since we admit that there have been changes, we must also assume some process of change which we may call progressive if by it we mean improvement—improvement in adaptation. This is probably what LeConte

*Loc. cit., p. 9.

meant. Evolution, then, is a process of change, each change being an improvement in adaptation to the environment, and resulting in an increase in complexity.

But this is not all evolution is. It is progressive change, but also change "according to certain laws." What are these laws? LeConte says: "I have been accustomed to formulate them thus: a. The law of differentiation; b. The law of progress of the whole; c. The law of cyclical movement."*

First let us consider the law of differentiation. Taking again the type form of evolution the development of the egg—first is the germ cell which as it divides and subdivides forms the egg. As cell division continues different aggregates of cells are set aside for different purposes: some form the ectoderm germ-layer which later forms the nervous system; others the entoderm layer from which is formed the nutritive system; and still others the mesoderm from which the circulatory system is formed. Thus there is an increasing differentiation among the cells resulting in greater specialization. Also, in the case of phylogenetic development, if we trace backward, we find that the various specialized types merge to form one branch, and the various branches one main trunk. "From such a common trunk, by successive branching and rebranching, each branch taking a different direction, and all growing wider and wider apart (differentiation), have been gradually generated all the diversified forms which we see at the present day. The last leafy ramifications—flower-bearing and fruit-bearing—of this tree of life, are the fauna and flora of the present epoch. The law might be called the law of ramification, of specialization of the parts, and diversification of the whole."†

His second law is the law of progress of the whole. LeConte assumes that there is a progress of the whole, but not necessarily a progress of all the parts of that whole. He seems to have a special idea of progress, namely, that progress is *up*—the passing of something to something else which is higher. He undoubtedly falls into this error by using the analogy of the tree. He speaks of some cells as advancing "to the dignity of brain cells," while "other cells descend to the position of kidney-cells." This seems to be a purely anatomical distinction

*Loc. cit., p. 11.
†Loc. cit., p. 13.

of higher and lower, and unwarranted from the point of view of development. What does he mean by the whole? Apparently we may mark off any unity we desire and call it "the whole." Thus within this whole are units each of which may be taken as a whole necessarily in progress, and within each of these other units and so *ad infinitum*. This being true there could be no degeneration, if degeneration is the opposite of progress. Thus, we are left in mid-air, so to speak, as to the meaning of his second law. He does, however, make one statement which may bring us on a more solid footing. "But here, also," he says in speaking of the specialization of different cells for various functions, "the highest cells are successively higher, and the whole aggregate is successively nobler and more complex." The last two words are the significant ones, for if he means by progress of the whole an increase in complexity, then we have here the same element which we found in the first part of his definition.

The last law is that of cyclical movement. Development is not uniform but in cycles. Each wave or cycle rises higher than the preceding one and then declines, and another wave comes and mounts still higher. The molluscs of the Silurian age dominated the earth, but they declined and gave place to the fishes which rose above the molluscs because of their greater complexity of structure and function. They too, however, declined and today hold a lowly position because the reptiles, mammals and man have successively reached points higher and higher than that of the fishes. These, LeConte calls, cycles of development. He applies this law to the development of society where he thinks it is quite conspicuous. He says:

> "Society everywhere advances, not uniformly, but by successive waves, each higher than the last; each urged by a new and higher social force, and embodying a new and higher phase of civilization. Again, as each phase declines, its characteristic social force is not lost, but becomes incorporated into the next higher phase as a subordinate principle, and thus the social organism as a whole becomes not only higher and higher, but also more and more complex in the mutual relations of its interacting social forces."*

This may be true, but it does not explain why social forces come in waves, nor why each is higher than the last. To

*Loc. cit., p. 26.

strike at the root of the matter we must know the meaning of that vague term "social forces." To say that the development of civilization happens thus and so does not explain it. We ask further, Why does it happen thus and so? Thus instead of finding a partial explanation in this law, we find it suggesting further questions.

Now that we have these three laws in mind, let us pause a moment and consider what LeConte means by the phrase, "according to certain laws." Of course, he is referring to the three laws which we have just discussed above. The question which arises, then, is, Do these three laws exhaust all the laws of evolution? Apparently LeConte thinks so for he does not mention any others. What, then, do we have thus far? The second law—progress of the whole—we have found bears little meaning; the third law—cyclical movement—is unimportant and practically negligible, as he himself admits, and so we have remaining only the law of differentiation, and "resident forces." This latter concept we shall now consider.

The third and important element of LeConte's definition is, "that the *forces* or causes of evolution are *natural;* that they reside in the thing developing and in the reacting environment."* The important thought, then, is that the forces are *natural* and *resident.* This is true, LeConte says, of embryonic development—the forces in the embryo are natural and they are resident. Is this true of all evolution? First we must see what he means by these two terms, "natural" and 'resident." In discussing these he says:

> "Before stating the two opposite views of the cause of evolution, it is necessary to remind the reader that when the evolutionist speaks of the forces that determine progressive changes in organic forms as *resident* or *inherent*, all that he means, or ought to mean, is that they are resident in the same sense as all natural forces are resident; in the same sense that the vital forces of the embryo are resident in the embryo, or that the forces of the development of the solar system according to the nebular or any other cosmogonic hypotheses are resident in that system. In other words, they mean only that they are *natural*, not supernatural. This does not, of course, touch that deeper, that deepest of all questions, viz., the essential *nature and origin of natural forces;* how far they are independent and self-existent, and how far they are only modes of divine energy."†

*Loc. cit., p. 28.

†Loc. cit., pp. 28–29.

Thus we are to understand that "resident" and "natural" forces are one and the same. From this quotation, we might be led to believe that LeConte is presuming too much; but when we turn to the third part of his book—*The Relation of Evolution to Religious Thought*—we find a clearer statement of his concept.

In discussing this point of natural law he says:

"It is the bringing together and complete reconciliation of the two apparently antagonistic and mutually excluding views of *direct agency* and *natural law*. Such reconciliation we have already seen is the true test of a rational philosophy. It is the belief in a God not far away beyond our reach, who once long ago enacted laws and created forces which continue of themselves to run the machine we call Nature, but a God *immanent*, a God resident *in* Nature, at all times and in all places directing every event and determining every phenomena. . . . . According to this view the phenomena of Nature are naught else than objectified modes of divine thought, the forces of Nature naught else than different forms of one omnipresent divine energy or will, the laws of Nature naught else than the regular modes of operation of that divine will, invariable because He is unchangeable. According to this view the law of gravitation is naught else than the mode of operation of the divine energy in sustaining the cosmos—the divine method of sustentation; the law of evolution naught else than the mode of operation of the same divine energy in originating and developing the cosmos—the divine method of creation; . . . . In a word, according to this view, there is no real efficient force but spirit, and no real *independent* existence but God."*

Thus God is immanent in Nature; He is the resident and natural forces which account for evolution. Evolution is His method of creation. Thus, when LeConte says, "by means of resident forces," he means God, Divine Will or Vital Force. To merely state that evolution is "due to resident forces" adds nothing to the elucidation of the *nature of the process* of development. Thus although LeConte's statement may be perfectly true, it does not go far enough; it does not answer our questions nor solve our difficulties. We wish to penetrate further into the problem and discover, if possible, the nature of these resident forces, why they are resident, and how they have produced that which is in evidence.

Let us sum up LeConte's definition of evolution as we have interpreted it. Evolution is change, in the direction of greater complexity, and differentiation due to a Vital Force. This is

*Loc. cit., pp. 300–301.

apparently all that we have left that is meaningful. Thus we see that there is nothing expressed in this definition which was not expressed by other thinkers before LeConte; he merely brings some of these thoughts together to form one concept. Change, we know was suggested by Heraclitus; the idea of complexity we find in Lamarck, Darwin and the Neo-Darwinians; differentiation was emphasized by Spencer, and Vital Force or God is a religious concept which we find in the history of evolutionary thought as early as Aristotle. Thus as we come to the close of our discussion of LeConte's contribution to evolutionary thought, we realize that it is rather significant that he wrote at the close of the nineteenth century for he epitomized the major ideas concerning evolution which were presented by thinkers who came before him. LeConte significantly marks the close of a long period in the history of evolutionary thought.

OBSERVATIONS ON *CERCARIA GORGONOCEPHALA* WARD.

Stephen R. Williams,
Miami University, Oxford, Ohio.

Since the Ohio State University Lake Laboratory moved to Put-in-Bay in 1918 there have been found each summer specimens of the compound cercaria discovered in that locality in 1898 by Dr. H. B. Ward.

The primary host and the adult worm are not yet identified but the life history must run roughly like this: The snail Goniobasis is very probably the secondary host and from it the cercaria come, numbers at a time, and fuse together by their tails. The mass of cercariæ with each cercaria tail extending and contracting drifts about as a sort of living bait for some aquatic creature, either a secondary or the primary host from which the embryoes reinfect Goniobasis.

There is no evidence of any purposive locomotion, though the mass can change its position in the water. On a bright sunny day around noon, if the water is quiet, a half hour of surface towing from the Gibraltar dock will usually yield one or two specimens. By inference one would assume that on a cloudy day they might be found at a little depth.

The evidence for Goniobasis is as follows: About the first of August, 1920, as the session was closing, Dr. F. H. Krecker called me over to see a Goniobasis which he had removed from the shell. As I looked through the microscope he jabbed the kidney region of the snail with a needle and out of the opening there sprayed numerous cercaria in a way that I can compare only to a "flower-pot" fire work as it is discharging. These cercaria had sticky tails and would attach to a needle or scalpel which touched the tails. I could see no accumulation of these artificially freed specimens into knots, however.

When ripe the cercaria must be released from the snail in groups or waves and as they writhe about the tails collide and stick, until balls of 25 to 60 individuals are formed. Fig. 1, photograph.

The cercaria tails are made up of two parts, the distal portions glandular, (Fig. 2) as a whole or in part, and the proximal parts strongly extensible. As long as the cercariæ are in place on the tails they are kept in constant motion, some tails contracting while others are extending, which makes the specific name very realistic.

The longer such a group drifts, the fewer cercariæ remain, and when all are lost the quiet ball of permanently contracted tails may be found at the bottom of the container.

FIG. 1.
Cercaria gorgonocephala. Photo × 25.

FIG. 2.
Photo of diagram based on camera sketch.

On July 30, 1929, a specimen was taken from the surface of the harbor by the dock which had a bubble of air enclosed by the free ends of the glandular portions of the tails. (Fig. 2). As a result the whole combination was floating about on the surface of the water much as a Portuguese man-of-war may float, and the cercaria were being thrust downward and outward as they extended. It was difficult to keep the creatures under observation because the breath of the observer carried the mass out of the field of the microscope.

The compound specimen photographed in figures 1 is less than 2 mm. in diameter as contracted. Single specimens are shown in figures 3 and 4 and the tip of the tail from which the

animal has been lost in figure 5. The separate nuclei show rather plainly in these specimens. The preserved animal is less than 0.2 mm. in length and half that in width at the widest part. Figure 4 is put in to show the point on the dorsal side at which the tail is attached.

FIG. 3. Ventral view, one cercaria.
FIG. 4. Lateral view, one cercaria and attachment to tail.
FIG. 5. End of tail with cercaria lost.

Whatever the balance of the life history, this grouping of the cercaria into a mass, whether floating or submerged, and the constant motion of the separate parts makes an adaptation for infecting the next aquatic host as a part of its food that can hardly be surpassed.

A SECTIONING CHAMBER.

Stephen R. Williams,
Miami University.

All who have had to do with paraffin sectioning in a small and crowded laboratory know the disadvantages. When the microtome is in use windows may not be opened, no one may walk rapidly across the floor in the vicinity, at times even loud speaking may be disastrous.

A Sectioning Chamber.

We have a laboratory, 20 feet by 22, in which we try to take care of the class in histology and embryology. Thus far the highest number of students registered has been twenty. By putting in a third laboratory period during the week and alternating some students on the two regular days it is possible to furnish each student a place to work.

The common paraffin bath and the microtome are in this room. The difficulties connected with the use of paraffin

in a single bath by so many students are great and are endurable only as long as consideration is shown by the users for each other.

The microtome difficulty has been to a great degree eliminated by the use of a chamber three feet ten inches square and six feet nine inches in height, constructed with glass on all sides as shown in the cut. One side has double sliding doors, twenty-one inches wide, which overlap each other when closed and which may be so pushed as to make an entrance either front or back. There is a heavy shelf running around two sides of the compartment at the level of the glass. On this shelf is clamped the microtome and it gives room for the box for the paraffin ribbon and for the heater mentioned below. Beneath the microtome shelf is a large ventilator, 18 inches by 8 inches, and in the roof there is another, 12 inches square.

When the doors are closed a radiant heater (600 watts, because no lesser heat could be obtained) placed on the shelf and pointed toward the microtome will very quickly raise the temperature of the chamber to the point where paraffin will section if the outside room temperature is at all endurable. Usually the heater is cut off before actual sectioning takes place and its connection is replaced by that to an electric lamp on the roof of the chamber.

Thus far no student knows by experience anything about that frictional electricity which sometimes cleaves a paraffin ribbon into its component sections and causes them to flit in all directions.

Due to the amount of glass used in the construction of the compartment, the laboratory room is not at all darkened by it.

It is also a protection against dust.

I wish I might boast that I had thought out this laboratory improvement, but it must be confessed that the chamber was made for an entirely different purpose for another department and then discarded. It took ten years of seeing it in a lumber room to envisage its function.

The convenience of being able to use the microtome at almost any time, either in the cold of winter with a stiff outside breeze, or in the warmer weather with the laboratory windows wide open, makes this a very valuable laboratory adjunct, worth all it costs either in space or appropriation.

DIURNAL VARIATIONS IN THE AMOUNT OF DISSOLVED OXYGEN, ALKALINITY, AND FREE AMMONIA IN CERTAIN FISH PONDS AT FAIRPORT, (IOWA).

A. H. Wiebe,
U. S. Bureau of Fisheries, Fairport, Iowa.*

INTRODUCTION.

Some preliminary observations made during the summers of 1927 and 1928 (Wiebe, 1930) made it apparent that the changes in the amount of dissolved oxygen and the alkalinity that occur in a pond within a 24-hour period are as large if not larger than the seasonal change where, for instance, the samples are taken regularly at 8 A. M. For this reason it was decided in 1929 to make a series of tests to determine the extent of the diurnal variations in dissolved oxygen, alkalinity, and in some instances ammonia nitrogen. Diurnal changes in the gaseous constituents of river water have been studied by Butcher et al. (1927–1928).

RESULTS AND DISCUSSION.

The first series was made on a pond called D-10 that has an area of 3.54 acres, on June 6–7, 1929. The water had a depth of 7.5 at the place of sampling. A small amount of plankton was present in the pond. The sky was clear during the day. The results for this series are shown in Table I. This table shows that the variations in the absolute amount of dissolved oxygen were relatively small. The percent of saturation on the surface rose from 105.53% at 8 A. M. to 125.76% at 4 P. M. It then decreased to 102.39% at 5 A. M. the next day. On the bottom the percent of saturation rose from 94.64% at 8 A. M. to 107.69% at 4 P. M. At 5 A. M. on June 7, it was down to 90.47%. The variations in free ammonia nitrogen were very small. The water at the surface was alkaline to phenolphthalein throughout the entire test period. This means there was a deficiency in free CO_2. This deficiency ranged from 2.26 p. p. m. to 5.65 p. p. m. On the bottom there was

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some free CO_2 at all times. This varied from a minimum of 1.13 p. p. m. to a maximum of 5.65 p. p. m.

TABLE I.

Table I Shows for D-10 Dissolved Oxygen, free CO_2, and Ammonia Nitrogen in Parts per Million. Percentage of Saturation for Dissolved Oxygen are also Shown.

Date	O_2		Percent Saturation		NH_3	N	Free CO_2	
	Top	Bottom	Top	Bottom	Top	Bottom	Top	Bottom
6–6–29								
8 A. M.....	10.08	9.18	105.53	94.64	.054	.052	—2.26	2.26
11 A. M.....	10.80	9.72	108.67	98.94	.054	.050	—3.39	2.26
2 P. M.....	10.73	.....	122.98	.....	.052	.052	—5.65	1.13
4 P. M.....	10.86	10.11	125.76	107.69	.....	.....	—5.65	1.13
7 P. M.....	10.56	9.93	120.00	105.00	.....	.....	—5.65	2.26
10 P. M.....	9.57	9.96	108.72	105.07	.052	.051	—4.52	2.26
6–7–29								
1 A. M.....	9.51	.....	104.73	.....	.....	.....	—4.52	.....
5 A. M.....	9.39	8.58	102.39	90.47	.050	.050	—2.26	5.65

TABLE II.

Table II Shows for D-3 Value for Dissolved Oxygen, Phenolphthalein Alkalinity and Ammonia Nitrogen in Parts per Million. Percentage of Saturation for Dissolved Oxygen are also Shown.

Date	Dissolved Oxygen		Percent Saturation		NH_3	Nitrogen	Phenolphthalein Alk.	
	Top	Bottom	Top	Bottom	Top	Bottom	Top	Bottom
7–2–29								
8 A. M.....	10.22	8.84	114.70	97.35	.052	.040	30.51	27.12
11 A. M.....	11.34	10.78	134.12	118.72	.028	.028	31.64	27.12
2 P. M.....	11.52	12.64	142.74	150.83	.025	.025	31.64	39.55
5 P. M.....	11.90	14.87	151.68	189.90	.032	.032	33.90	37.29
8 P. M.....	11.15	13.76	131.87	167.39	.026	.028	31.64	33.9
12 P. M.....	11.34	12.83	132.94	153.10	.032	.026	.....	.....
7–3–29								
2 A. M.....	12.64	.....	148.18	.....	.....	.....	.....	.....
3:30 A. M...	12.64	11.15	145.62	128.45	.032	.032	.....	.....
5:30 A. M...	11.52	7.99	130.46	87.99	.....	.....	.....	.....
9:00 A. M...	11.90	10.59	135.92	116.63	.027	.028	.....	.....

Table II gives the result for a series of determinations made on a pond called D-3 that has an area of approximately 0.9 acre and a depth of 5 feet at the sampling point. This series was run on July 2–3, 1929. This pond had a good crop of Aphanizomenon at this time. It was approaching the state

of water bloom. The dissolved oxygen in the surface samples varied from 10.22 p. p. m. to 12.64 p. p. m. and the percent of saturation from 114.70% to 151.68%. In the bottom samples the dissolved oxygen ranged from 8.84 p. p. m. at 8 A. M. July 2, to 14.87 p. p. m. at 5 P. M. At 5:30 the next morning it was down to 7.99 p. p. m. The percent of saturation ranged from 97.35% at 8 A. M. to 189.90% at 5 P. M. Then at 5:30 A. M.

TABLE III.

Table III Shows Values for C-2 and C-3 for Dissolved Oxygen, Phenolphthalein Alkalinity in Parts per Million. Percentages of Saturation for Dissolved Oxygen are also Shown.

Date	Dissolved Oxygen	C-2 Percent Saturation	Phenolphthalein Alk.	Dissolved Oxygen	C-3 Percent Saturation	Phenolphthalein Alk.
8–16–29						
8 A. M.	11.89	125.93	27.12	11.28	124.72	25.99
10 A. M.	13.93	156.90	28.25	13.68	154.21	29.38
12 N.	.....	.....	36.16	19.22	223.35	41.80
2 P. M.	20.18	240.81	44.07	21.98	262.29	44.07
3 P. M.	20.42	243.67	45.2	25.94	312.65	51.98
4 P. M.	21.74	259.42	45.2	24.85	297.67	57.63
6 P. M.	19.70	233.76	48.59	24.25	289.37	61.02
8 P. M.	.....	.....	47.46	22.69	258.96	57.63
10 P. M.	18.98	216.66	42.94	20.19	230.61	51.98
8–17–29						
1 A. M.	17.18	191.69	40.68	18.24	204.70	42.94
2 A. M.	14.89	167.11	38.42	17.29	192.32	41.80
3 A. M.	14.41	161.40	38.42	16.09	178.97	.....
4 A. M.	14.41	161.40	36.16	15.61	173.63	42.94
5 A. M.	13.09	144.16	36.16	13.81	152.09	.....
6 A. M.	11.65	128.30	32.77	13.10	143.42	32.77
7 A. M.	13.69	151.37	32.77	14.64	161.23	35.03
8 A. M.	13.89	154.50	36.16	16.33	181.64	39.55

July 3, the percent saturation was down to 87.99%. The ammonia nitrogen showed the largest change between 8 and 11 A. M. After that the changes were relatively small. Alkalinity determinations were made from 8 A. M. to 5 P. M. The water was always strongly alkaline to phenolphthalein, showing that a free CO_2 deficiency existed. The changes in alkalinity were small in the surface samples. In the bottom samples the alkalinity varied from 27.12 p. p. m. at 8 A. M. to 39.55 p. p. m. at 5 P. M. At 8 P. M. it was down to 33.9 p. p. m.

The fact that greater variations in dissolved oxygen and alkalinity occurred in the bottom samples than in the surface

samples is accounted for by a peculiar habit of the alga, Aphanizomenon. This alga reacts negatively to strong sunlight. So on a clear day after the sun gets up fairly high in the sky this alga begins to move downward. During the night it migrates again into the upper strata. This is what happened on the day when this series of tests was run. This behavior of Aphanizomenon, I think, accounts also for the rise in the dissolved oxygen at the surface during the night.

Table III presents the results of a series of tests made on the waters of two small concrete ponds. These ponds, C-2 and C-3, each have an area of 378 square feet. The depth of the water is 14 inches at one end and 20 inches at the other. These ponds were in a state of water bloom consisting of a number of species of green and blue-green algæ. The algæ in these ponds were so thick that the transparency of the water was reduced to a few inches. Only surface samples were taken in this case. The percent of saturation in C-2 ranged from 125.93% at 8 A. M. August 16, 1929, to 259.42% at 4 P. M. on the same date. At 6 o'clock the next morning the percent of saturation was down to 128.30%. In C-3 the percent of saturation of dissolved oxygen varied from 124.72% at 8 A. M. to 312.65% at 3 P. M. on the same date. The following morning at 6 o'clock it was down to 143.42%. The water in both ponds was strongly alkaline throughout the entire test period. This again shows the entire absence of free CO_2. In C-2 the range in alkalinity was from 27.12 p. p. m. to 48.59 p. p. m. Then it decreased to 32.77 p. p. m. In C-3 the changes were from 25.99 p. p. m. to 61.02 p. p. m. and then to 32.77 p. p. m.

Table IV shows another set of determinations made on the ponds discussed in the preceding paragraph. In this case samples were taken from the bottom as well as from the surface. The condition of water-bloom still persisted. At 8 A. M. September 4, the percent of saturation with O_2 amounted to 88.9% at the surface of C-2; at 4 P. M. this had risen to 267.90%. The next day at 8 A. M. it stood at 52.34%; at 4 P. M. it was up to 254.72%. At 8 A. M. on September 6, it was down to 114.45% again. The table shows that large changes in the percent of saturation also occurred in the bottom samples. The phenolphthalein alkalinity in the surface samples of C-2 ranged from 23.73 p. p. m. to 67.80 p. p. m. to 32.77 p. p. m. to 62.15 p. p. m. and finally to 45.2 p. p. m. Large changes

FIGURE 1.

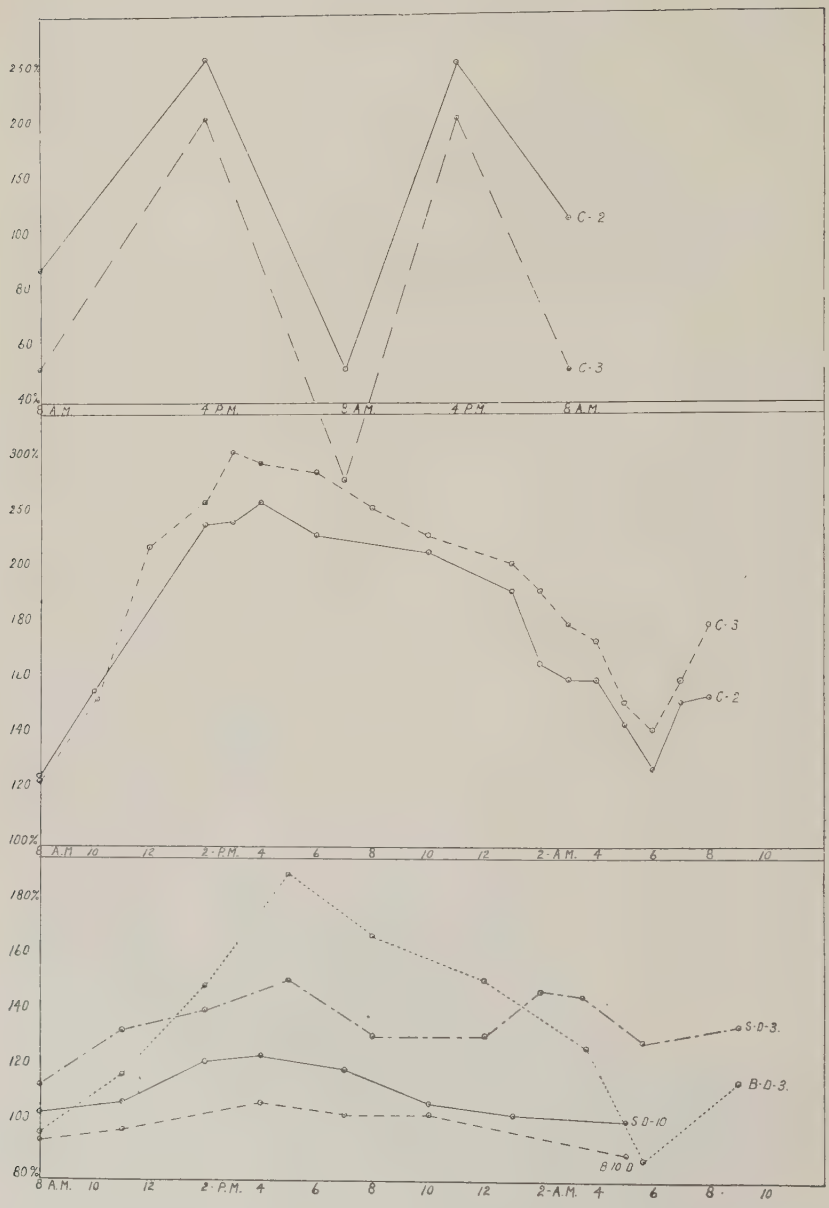

FIG. 1. Variations in the percentage of saturation of dissolved oxygen. C and D indicate pond series; S stands for surface and B for bottom.

occurred also in the alkalinity of the bottom samples in C-2. The results for C-3 are essentially similar to those for C-2, i. e., the O_2 and the alkalinity increased during the day and decreased during the night. The variations in the percentages of saturation with dissolved oxygen in D-10, D-3, and for the first two sets of experiments on the C-ponds are shown graphically in Fig. 1. The graphs were prepared by Mr. J. B. Southall.

TABLE IV.

Table 4 Shows Values for Dissolved Oxygen, Phenolphthalein Alkalinity in Parts per Million. Percentages of Saturation are also Shown.

Date	Dissolved Oxygen		Percent Saturation		Phenolphthalein Alkalinity	
	C-2	C-3	C-2	C-3	C-2	C-3
9–4–29						
8 A. M.	7.85	4.36	88.9	52.04	23.73	19.21
	5.93	1.91	70.12	22.59	22.60	19.21
5 P. M.	21.62	16.92	267.90	205.83	67.80	67.80
	15.34	11.68	188.33	140.72	54.24	54.25
9–5–29						
8 A. M.	4.70	1.06	52.34	11.55	32.77	14.69
	4.02	1.06	44.27	11.67	33.90	16.95
4 P. M.	21.11	17.62	254.72	204.76	62.15	54.24
	19.88	7.66	229.03	86.74	64.41	39.55
9–6–29						
8 A. M.	10.81	4.89	114.45	51.76	45.2	30.51
	11.34	5.06	120.44	54.11	45.2	19.21

NOTE.—In Table IV, the upper figures in each case refers to the surface sample. The lower figures has reference to a bottom sample.

Table V shows the results of some determinations made on C-3 on August 19, 1929. This table shows in addition to the dissolved oxygen and phenolphthalein alkalinity also the methyl orange alkalinity, pH and temperatures. The fact of particular interest here is that at 1 P. M. the phenolphthalein alkalinity equalled one-half the methyl orange alkalinity, and at 4 P. M. the methyl orange alkalinity was less than twice the phenolphthalein alkalinity. This means that the algæ had used all the free CO_2, all the bicarbonate CO_2 and some of the CO_2 of the normal carbonate. This means that the alkalinity was due to the normal carbonate and the hydroxide. It had previously been reported by Birge and Juday (1911) that certain algæ used as much as 83% of the half-bound CO_2 and by Wiebe

(1930) that algæ used 92% of the half-bound CO_2. That algæ can use all of the half-bound and some of the bound is in accord with the observation of Neresheimer and Ruttner (1929) that aquatic plants may withdraw small amounts of CO_2 from normal carbonates.

TABLE V.

Table V Shows Dissolved Oxygen, Phenolphthalein Alkalinity, Methyl Orange Alkalinity in Parts per Million, Ph Values and Temperatures in Degrees C. for C-3 on August 19, 1929.

Hour	O_2	Phenol-phthalein Alk.	M. O. Alk.	Ph.	Temp.
8:30 A. M.....	12.01	40.68	138.99	9.3	20.75
1:00 P. M.....	18.25	68.93	137.86	9.5	27.5
4:00 P. M.....	27.63	76.84	137.86	9.5	27.5

SUMMARY.

(1) The diurnal variations in dissolved oxygen, phenolphthalein alkalinity, and free ammonia nitrogen have been determined in several instances.

(2) Very marked changes in the actual amount of dissolved oxygen and phenolphthalein alkalinity were observed within a 24-hour period.

(3) The extent of these variations are determined by the abundance of the algæ.

(4) It appears that the algae can utilize all the CO_2 present as the bicarbonate as well as some of the CO_2 of the normal carbonate.

(5) Table IV suggests that the algae may become so abundant as to endanger fish life by depletion of the O_2 during the night.

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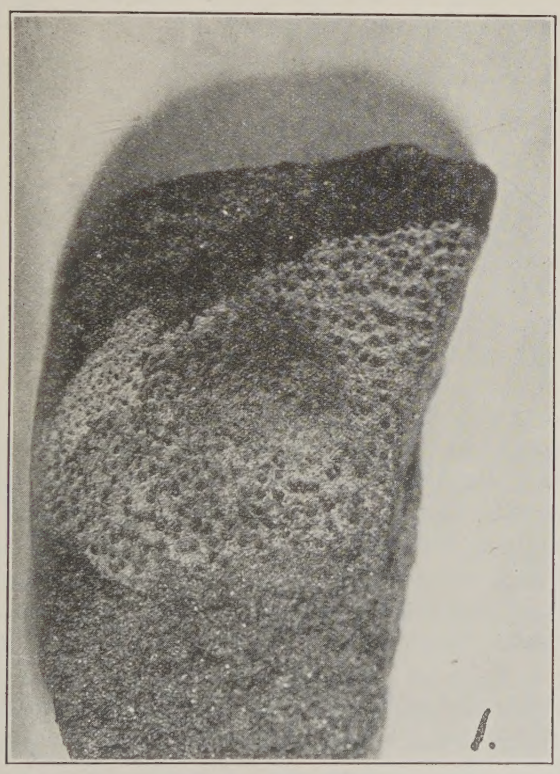
A NEW CRUSTACEAN OF THE FAMILY AGLASPIDÆ FROM THE UPPER MISSISSIPPI VALLEY.

WILLIAM A. P. GRAHAM,
Ohio State University.

Aglaspis major n. sp.

DESCRIPTION.—The specimen consists of a very thin portion of the carapace of a large aglaspid in which the left eye is preserved. The



Aglaspis major Graham.
FIG. 1. Cast of cephalothorax showing eye spot; × 2½.

specimen consists of a whitish film penetrated by a large number of small openings through which the yellow matrix of the enclosing rock may be seen. The film is thought to be the residue of a test which was perforated by numerous holes. The holes are interpreted as being tangential sections through these perforations.

Cephalothorax apparently broad, with large circular eyes situated near the antero-marginal borders. The eye of the specimen is located

on a prominent node from which the surface slopes gradually to the margin of the cephalothorax and steeply toward the mid-line of the head. The eye node rises to a maximum elevation of two millimeters above the general surface. It is eight millimeters long measured parallel to the median line of the head, and nine millimeters wide measured at right angles to the median line. The cephalothorax is perforated by numerous holes or pits which are about one-half millimeter in diameter on the cephalothorax proper and grade down to mere pin points on the border. They are arranged in roughly concentric fashion outward from the eye. The average number of pits per square centimeter is thirty-six, being more numerous on the border area where they are smaller than on the main part of the head. (Figure 1).

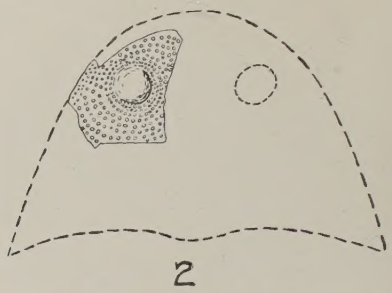


FIG. 2. Reconstructed cephalothorax, ⅔ natural size.

REMARKS.

This specimen was found in association with abundant trilobite and brachiopod remains. As far as known it represents the only aglaspid ever found in this locality and the largest one yet discovered in the Croixan of the Upper Mississippi Valley. On the basis of this fragment the head was probably three inches wide and two inches long and the reconstructed animal would probably be from six to eight inches long. (Figure 2).

The specimen resembles somewhat *Aglaspis eatoni* Whitfield, but differs by being much larger in size, by having larger, more rounded eyes which are located nearer the margin of the carapace; and in the occurrence of a number of large, strongly marked holes or pits arranged in more or less distinct concentric rings around the eyes.

POSITION AND LOCALITY.

Upper Cambrian (Croixan), Eau Claire sandstone, Cedar Falls, Wisconsin. The specimen was found in a one and one-half to two-inch bed of fine-grained, buff, micaceous sandstone, two and one-half feet above the average level of Red Cedar River, about a quarter of a mile below the dam.